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Declining Unions and the Coverage Wage Gap: Can German Unions Still Cut It? †

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That German trade unionism is in decline seems to be beyond dispute. More controversial is the implied change in union impact on worker wages. A linked employer-employee dataset is deployed over an interval of continuing decline in unionism to address this issue. Over the sample period 2000-2010 it is found that joining a sectoral agreement always produces higher wages, while exiting one no longer leads to wage losses if the transition is to a firm agreement. Leaving a firm agreement to non-coverage also leads to wage reductions, while joining one from non-coverage appears decreasingly favorable. The one constant is the persistence of a small positive union wage gap.

1. Introduction

Most information on the union-nonunion differential in Germany – strictly, the wage gap between covered and uncovered workers – pertains to the 1990s or early 2000s. Yet, as is widely known, German unions have been in retreat both during and subsequent to these intervals. It is the very breadth of this decline that makes investigation of the more recent interval more compelling.

First, union density has fallen sharply. The decline can be dated from the mid-1980s, the sudden rise in membership after 1989 proving to be little more than a diversion. Union density declined from 36 percent in 1991, in the aftermath of unification, to 19.3 percent in 2009 (see Addison, Schnabel, and Wagner, 2007; Bispinck, Dribbusch, and Schulten, 2010; Fitzenberger, Kohn, and Wang, 2013).[1] Second, overall collective bargaining coverage as a share of employment fell in western Germany (eastern Germany) from 70 (55) percent in 2000 to 65 (51) percent in 2009 (see Bispinck, Dribbusch, and Schulten, 2010). The corollary was a continuing rise in the bargaining free sector. Third, the decline in collective bargaining coverage was not deflected by a growth in *orientation* in the uncovered sector, nor was it to receive any support from the *extension* principle.

Orientation refers to a process whereby uncovered firms claim to shadow the terms of sectoral agreements. The coverage of orienting firms in the private sector rose from 17.9 percent of employment in 2000 to 22.4 percent in 2010. This rising trend only partially compensated for the decline in *sectoral* bargaining, since the share of employees covered by sectoral agreements fell from 59.9 to 49.3 percent over the same interval. Moreover, these are simple frequencies. The wages paid by orienting firms have been shown to lie well below those set under collective bargaining (see Addison et al., 2014). As far as the extension of collective agreements to employees and employers not bound by the relevant sectoral agreement is

[1] Interestingly, Hirsch and Schnabel's (2011) measure of union bargaining power, based on a right-to-manage model of collective bargaining, suggests that the fall off in union strength occurred mostly after 1999, remaining fairly stable in the 1990s despite a fairly uniform drop in density over the entire period.

concerned this, too, has evinced pronounced decline. For example, considering just the extension of primary collective agreements under the 1949 Collective Agreement Act, their number fell from 408 (or 5.4 percent of all such agreements) to just 245 (1.5 percent) in 2009 (Bispinck, Dribbusch, and Schulten, 2010).

Finally, German sectoral collective bargaining has been buffeted by decentralizing tendencies, as manifested in opening clauses and in-plant alliances or pacts for employment and competitiveness (see, for example, Bispinck, 2004; Seifert and Massa-Wirth, 2005). These extensive contractual innovations have allowed firms to deviate from sectorally-agreed provisions (see, inter al., Haipeter and Lehndorff, 2009). Their deployment has occasioned a considerable debate in the industrial relations literature, initially between those emphasizing change (erosion, disorganization, liberalization, and neoliberalism) and those emphasizing continuity (path dependence, hybridization, and varieties of capitalism) and, latterly, between those who discern stability in a shrinking core and those who see the decline in coverage as corrosive of the system as a whole (see, respectively, Hassel, 1999, 2014; Streeck, 2010).[2]

The very scale of these changes in Europe's largest economy makes Germany an interesting case for consideration. The focus of the present article upon the union wage gap is justified because of the sparse nature of research into the union premium, disagreement as to its magnitude and, to repeat, lack of information on its development.[3] Against this backdrop, the present exercise which seeks to obtain estimates of the course of the union wage gap at a

[2] Economists have generally eschewed formal analysis of the decline in coverage – exceptions include Addison et al. (2011) and Fitzenberger and Sommerfeld (2014) – although a popular view is that collective bargaining decline has its counterpart in massively improved economic competitiveness (e.g. Dustmann et al., 2014).

[3] The key articles in the literature are Stephan and Gerlach (2005), Antoncyzk (2010), Gürtzgen (2012), and Fitzenberger, Kohn, and Lembcke (2013).

time of unambiguously declining union authority while controlling for unobserved firm and worker heterogeneity gains additional purchase.

The unique dataset used in this inquiry is the cross sectional version of the linked employer-employee data supported by the German Institute for Employment Research. The analysis is based on two (three-year) clouds of data at each end of the sample period 2000-2010, exploiting changes in establishment collective bargaining status over time. Estimates of union wage effects are obtained by comparing the wage growth of workers employed by plants joining or leaving collective bargaining with that of workers employed by establishments that did not change their collective bargaining status – designated as never members and always members, respectively, which counterfactuals are then reversed for robustness checks.

It is reported that joining a sectoral agreement always yields higher wages, while leaving one no longer produces wage losses if the transition is to a firm agreement. Leaving a firm agreement to non-coverage also leads to wage reductions, while joining one from non-coverage seems decreasingly favorable. The reverse counterfactuals in turn yield correspondingly smaller estimates (in absolute value) of wage development than reported for the initial counterfactuals.

2. The Dataset

The study uses the LIAB Cross-Sectional Model Version 2 1993-2010 (LIAB QM2 9310) of the linked employer-employee data supported by the Institute for Employment Research/*Institut für Arbeitsmarkt- und Berufsforschung* (IAB). The LIAB data are created at the Research Data Center (*Forschungsdatenzentrum*, or FDZ) of the Federal Employment Agency/*Bundesagentur* *für Arbeit* by linking the establishment data from the annual waves of the IAB Establishment Panel (*Betriebspanel*) with information on individuals from the social security records of the German Federal Employment Agency.

The IAB Establishment Panel is a large-scale annual establishment survey that covers up to 16,000 establishments every year, beginning in 1993 in West Germany and extended in 1996 to the former East Germany. The participating establishments are surveyed on a large number of employment policy-related subjects. These include employment development, business policy and business development, collective bargaining, personnel structure and recruitment, remuneration, and working time. The survey is unique in Germany, since it is representative for all industries and establishment sizes nationwide and was conceived from the outset as a longitudinal survey.

The information on individual workers in the LIAB dataset comes from the social security records of the German Federal Employment Agency and covers all employees of the establishments surveyed in the IAB Establishment Panel. Specifically, it includes employees who are subject to social security and also those who are marginally part-time employed (*geringfügig beschäftigt*). For these employees, several demographic characteristics such as gender, age, nationality, level of education, occupational group, employment status and place of residence are provided. Furthermore, the data contain the individual daily wage of an employee. The latter is measured with high accuracy by the authorities since this wage information is decisive in calculating an individual's social security payments.

In sum, the LIAB dataset is a unique data source for analysing both the supply side and the demand side the German labor market along various dimensions. Due to its coverage, it is also one of the best-suited datasets for investigating the effects of collective bargaining coverage on the wages of individual workers in Germany. Several versions of the LIAB data including cross-sectional and longitudinal subsamples can be accessed for scientific purposes at the FDZ in Nuremberg.

Several modifications were made to adapt the data to research needs. First, in order to improve the quality of the linkage between the survey data and the administrative data, the procedure followed by the FDZ in some longitudinal versions of the LIAB for erasing observations that exhibit bad linkage quality was followed. A link is defined as having a bad quality if the number of employees and apprentices that an establishment has reported in the IAB Establishment Panel deviates significantly from the number of employees and apprentices that is calculated from the administrative data. (For information on this procedure, see Jacobebbinghaus, 2008: 53ff.)

Second, other modifications concern the key wage variable. In the LIAB data, the reported individual wage of a worker is the gross daily wage. Since there exists an upper contribution limit in the German social security system, the gross daily wages are top-coded, affecting 15 (10) percent of the observations for western (eastern) Germany in the dataset. Wages above the contribution limit were imputed, using the procedure suggested by Gartner (2005). This involved first estimating a Tobit regression of log daily wages on individual and establishment characteristics separately for both parts of the country and for each single year, next constructing a truncated normal distribution by using the predicted values from the Tobit estimation as moments and by setting the lower truncation point equal to the contribution limit, and finally by replacing censored wage observations by values randomly drawn from this truncated normal distribution. For their part, wages were deflated using the Consumer Price Index for 2000.

Third, because only a very broad measure of individual working hours is contained in the dataset – in particular, for part-time workers, whether working hours are less or greater than 18 per week – the analysis was restricted to full-time employed workers who are subject to social security. We further excluded those full-time workers who were recorded as receiving an implausibly low daily wage (of less than €16). In addition, we excluded observations from the following sectors/enterprises: agriculture, hunting, fishing and forestry, public administration, and not-for-profit entities.

Note that use of the cross-sectional version of the LIAB precludes tracking a worker if he/she leaves one establishment for another that is not covered by the IAB Establishment Survey, or if he/she exits to non-employment. The same problem arises if a worker remains in an establishment that subsequently, and for whatever reason, no longer participates in the IAB Establishment Survey. Unfortunately, there is no way of circumventing this limitation of the data. Making use of one of the available longitudinal versions of the LIAB, or constructing an 'LIAB-variant,' would not suffice as the key information on the collective bargaining status of an establishment in a given year will not be available if the establishment fails to participate in the IAB Establishment Survey for that year.[4]

3. Preliminary Data Analysis

[4] For further information on the IAB Establishment Survey and the LIAB, see, respectively, Fischer et al. (2009) and Henning, Scholz, and Seth (2013).

The analysis of this article is based on two (three-year) clouds of data – annual observations for 2000-2002 and 2008-2010 – rather than the full 2000-2010 panel. If the entire sample period were used to calculate one- and especially two-year changes in status, etc., this procedure would over-emphasize the role of permanent establishments in the panel. The more selective approach followed here avoids results being based on workers who belong mainly to the large establishments that dominate the permanent panel. Further, this approach has the advantage of contrasting two distinct periods occurring at the beginning and at the end of the first decade of the present century, marked by a distinct decline in collective bargaining coverage and in union density.

Table 1 presents the main longitudinal features of each subset of observations. Three main observations are in order. First, each subperiod contains approximately the same number of workers: 2.4 million in 2000-2002 and 1.9 million in 2008-2010. Second, the share of individuals appearing in each and every year of each cloud is roughly the same: 29.9 percent in the first interval, 32.9 percent in the second. Finally, the percentage of individuals who are observed at least twice over the two intervals is also approximately constant at 50.9 and 55.8 percent of the total, respectively. The two clouds of data are therefore comparable in terms of the number of workers being observed and their longitudinal profile.

(Tables 1 and 2 near here)

The longitudinal pattern of individuals observed at least twice in each observation period is considered in Table 2. The issue is whether workers stay in the same establishment or switch employers. Clearly, in either case – workers who are observed in three consecutive years or just two years (consecutively or otherwise) – job stayers massively dominate in each sub-period. Accordingly, identification of the union wage effect in the LIAB data has to be based on the wage development of job stayers, in conjunction with observed changes in establishment collective bargaining status (see also section 4).

The frequency with which establishments switch their collective agreement status is indicated in Table 3. Both one-year and two-year transitions are reported. Switching is identified here by changing responses to the question in the IAB Establishment Panel inquiring of the firm respondent whether the establishment is covered by a collective agreement.[5] In the interests of economy, the table now aggregates firm and sectoral agreements into the single category of 'any type of collective agreement.' (A table containing transitions for the two separate collective bargaining arrangements is available from the authors upon request.)

(Table 3 near here)

Between 2000 and 2001, for example, it can be seen that 424 out of 3,639 establishments (or 11.7 percent) abandoned collective bargaining of either type while 441 out of 2,792 establishments (15.8 percent) joined a collective agreement from an initial state of no coverage. Over this interval, therefore, the percentage of collective bargaining switchers in the total number of possible cases is 13.5 percent (=[(424+441)/6,431]*100). So, a considerable share of establishments change their collective bargaining status, made up of roughly equal numbers of joiners and leavers. Approximately the same percentage of collective bargaining switchers is observed in 2001-2002, at 12.8 percent. For the years 2008-2009 and 2009-2010,

[5] There is some inevitable imprecision with respect to leavers – although not joiners – because under German law (the Nachwirkungsfrist doctrine) those leaving a (active) collective agreement are supposed to follow its terms for incumbent (but not new) employees until a new agreement has been reached at firm level or as a result of individual bargaining. It is assumed here that that the longer the firm respondent claims to be no longer covered by a collective agreement, the longer the establishment has left a collective agreement and the further adrift of current contracts are its wages.

the percentage of switchers is 11.5 and 6.8 percent, respectively. Two-year transitions are slightly higher than the one-year transitions, at 14.6 and 10.6 percent in the first and second sub-periods, respectively.[6]

Estimates of the collective bargaining wage gap were obtained by fitting an augmented Mincerian earnings function to the separate cross-sections of data. Specifically, the wage gap was conditioned on 24 (67) worker- (establishment-) level covariates. The former included gender, age (and its square), years of service (and its square), citizenship status, education (6 levels), and occupation (12 levels). The latter comprised dummies for location, establishment age, the profit situation, the state of technology, works council status, firm size (and its square), and industry (40 2-digit), together with the share of female, fixed-term, foreign and skilled workers, and employee median age.

(Table 4 near here)

Summary regression results are provided in Table 4. The material in the first column indicates a positive wage gap of 7 to 14 percent in favor of workers covered by sectoral agreements, relative to the comparison group of workers in non-covered establishments. This is a sizeable wage gap, consistent with some earlier OLS studies. Note the seeming upward trend of this wage gap. The evidence on sectoral versus firm-level agreements in the second column of the table points to a wage gap favorable to the former, of some 1.8 to 4.0 percent. The results in the final column point unequivocally to higher earnings under any form of collective

[6] Two-year transition rates would be substantially higher were the two samples the same. They are not because two-year transitions require establishments to be in the sample for three consecutive years, meaning that the sample will contain a materially higher proportion of permanent panel stayers.

bargaining than under individual bargaining, the margin amounting to some 6.3 to 12.5 percent.

4. Estimation strategy

Assume that the (log) gross daily wage for individual *i* in period *t*, y_{it}, is given by:

$$y_{it} = Z_{it}\beta + \delta U_{jt} + \lambda_t + \theta_i + \psi_j + \varepsilon_{it} , \tag{1}$$

where θ_i and ψ_j denote worker- and firm-specific time-invariant effects, respectively; Z_{it} is a vector of observed time-varying and time-invariant worker- and firm-level characteristics, as noted in section 3; λ_t is a time dummy; U_{jt} is a dichotomous variable indicating the collective agreement status of firm *j* (so that δ denotes the collective bargaining wage premium); and ε_{it} is the error term of the model. As is conventional, further that assume $E(\varepsilon_{it}|Z_{it}, U_{jt}, \theta_i, \psi_j) = 0$.[7]

Given that each observation window (i.e. 2000-2002 and 2008-2010) comprises a 3-year interval, one possible route controlling for worker and firm heterogeneity is to take a 2-year difference from model (1), to obtain:

$$y_{it} - y_{it-2} = (Z_{it} - Z_{it-2})\beta + \delta(U_{jt} - U_{jt-2}) + (\lambda_t - \lambda_{it-2}) + (\psi_{jt} - \psi_{jt-2}) + (\varepsilon_{it} - \varepsilon_{it-2}). \tag{2}$$

Clearly, in following this approach the aim is to capture some medium-term effect of collective bargaining coverage. Thus, using the sample of job stayers, for whom by construction $(\psi_{jt} - \psi_{jt-2}) = 0$, model (2) yields:

$$y_{it} - y_{it-2} = (Z_{it} - Z_{it-2})\beta + \delta(U_{jt} - U_{jt-2}) + (\lambda_t - \lambda_{it-2}) + (\varepsilon_{it} - \varepsilon_{it-2}). \tag{3}$$

[7] Note that applying OLS to model (1), as was done in Table 3 in a purely cross-section fashion, is equivalent to assuming away worker and firm unobserved heterogeneity – or, alternatively, that ω_{it} is not correlated with U_{jt}, where $\omega_{it} = \theta_i + \psi_j + \varepsilon_{it}$.

In other words, given that individuals stay in the same firm, identification of δ is achieved via workers whose establishments have changed their status from *t-2* to *t*.

For movers, on the other hand, in general $(\psi_{jt} - \psi_{jt-2}) \neq 0$. This means that under the assumption that $(\psi_{jt} - \psi_{jt-2}) + (\varepsilon_{it} - \varepsilon_{it-2})$ is uncorrelated with $(U_{jt} - U_{jt-2})$, an OLS regression of model (2) will give an alternative estimate of the effect of collective bargaining coverage. Identification of δ in this case is via job movers whose establishments in *t-2* and *t* have the same coverage status vis-à-vis job movers whose establishments have changed their status. Unfortunately, as was described in section 2, the number of job movers is too small to pursue this approach. The empirical strategy will therefore perforce rely solely on job stayers.[8]

Not wishing to impose symmetry on the effects of an establishment leaving/joining a collective agreement, implementation of a difference-in-differences approach is carried out by running the selected models across separate subsamples of establishments. These comprise collective bargaining leavers and always members on the one hand, and collective bargaining joiners and never members on the other.

Implementing a 2-year difference strategy for the two groups of leavers and always members, on the one hand, and joiners and never members on the other, and then regressing the changes in the wage outcome indicator on the corresponding change in collective bargaining status implicitly assumes that any macro shock, proxied by a time dummy, has a

[8] It would also be possible to use the raw annual data and run the spell fixed-effects version of model (1). In this case, by first-differencing within each spell (only consecutive observations on job stayers are useable for estimation), we have $\Delta\theta_i = 0$ and $\Delta\psi_j = 0$, and therefore model (1) becomes $\Delta y_{it} = \Delta Z_{it}\beta + \delta\Delta U_{jt} + \Delta\lambda_t + \Delta\varepsilon_{it}$, where Δ denotes the first difference operator. Results of a spell fixed-effects model are available from the authors upon request. Note further that computing 3- and 4-year differences, for example, would lead to a substantial reduction in the number of workers (as the number of establishments with four and five consecutive observations is much smaller than the number of establishments with two) as well as a sharply increasing proportion of large establishments in the total number of establishments with available data.

similar impact on both treated and control groups (e.g. leavers and always members, respectively) or that the macro shock does not have any differentiated impact on the decision to leave/stay covered by a collective agreement in this particular case.

Dropping either assumption entails proceeding beyond the ‘unadjusted’ difference-in-differences estimator, which is likely to rely upon even stronger assumptions. No attempt, then, will be made here to correct for the possibility that the macro effect is distinct over the treated and control groups. That said, an attempt will be made to check the robustness of the results reported here, using alternative control groups. Note that even if treated and untreated groups appear to have distinct observable characteristics, it does not necessarily follow that they will respond differently to a given shock; or, conversely, that a common set of characteristics will generate an identical propensity to change collective bargaining status.

5. Two-Year Differences

Wage formation is next placed in a longitudinal context, allowing for unobserved establishment and worker heterogeneity. Estimates of the collective bargaining premium in the two-year difference formulation, using the subset of job stayers, are presented in Table 5. Establishments are grouped into separate samples of sectoral and firm-level agreement leavers and joiners *and* their corresponding comparison groups of sectoral and firm agreement stayers (i.e. always members and never members), as indicated in the first four columns of the table. The table thus gives the 2-year effect of collective bargaining – either sectoral or firm-level agreements – on those individuals who do not switch jobs between *t-2* and *t* but who happen to be in establishments whose status has changed versus individuals whose establishments do not switch status. To repeat, identification of the union/collective bargaining effect is obtained via changes in an establishment's collective agreement status, given that workers stay in the same establishment over the selected interval.

(Table 5 near here)

As can be seen from the table, workers whose establishments leave a sectoral agreement for no coverage have their wages reduced by 0.7 percent over the 2000-2002 interval, compared with those whose establishments remain covered by a sectoral agreement. The corresponding effect for the 2008-2010 period is -0.4 percent. If, in turn, a firm *leaves* a sectoral agreement and becomes covered by a firm agreement, the effect is less pronounced in the first sub-period, at -0.4 percent, and eventually reversed in the second sub-period, at +0.8 percent.

The evidence from workers whose establishments have joined sectoral agreements is stronger than that found for sectoral agreement leavers, at +0.7 and +1.1 percent in 2000-2002 and 2008-2010, respectively, in the scenario where the initial state is of no coverage by any type of collective agreement. These gains are even larger if the transition is from firm-agreement coverage, at +1.0 and +2.3 percent, respectively. Note that the latter result seems to contradict the evidence found for sectoral agreement leavers in the second row, last column, in the sense that from the perspective of worker wages it looks equally possible to have higher wages either from switching from sectoral to firm agreements or the other way around. Our preferred explanation for this apparent contradiction is that two sets of estimates might not be extracted from strictly comparable samples (see below).

The remaining four rows of the table examine the transitions between any type of coverage and firm agreements, on the one hand, and between any type of collective bargaining coverage and no coverage at all, on the other. Thus, in the fifth row, there is a reduction in wages for those workers whose establishments left a firm agreement to become 'uncovered,' at -0.9 and -0.7 percent in the two selected intervals, respectively; while joining a firm agreement from no coverage, in the sixth row, is increasingly less favorable to worker wages, at 2.4 percent in 2000-2002, and 0.2 percent (and statistically insignificant) in 2008-2010. Again, the wage effects – extracted from switchers into firm agreements from the non-coverage regime and vice-versa – are asymmetric. This lack of symmetry returns us to the point that our approach is quasi-experimental in nature and not a truly experimental exercise.

The last two rows offer perhaps more clear-cut results. Here the comparison is between a situation of no coverage with any type of collective bargaining coverage. The evidence strongly points to a negative effect on wages of leaving a sectoral or firm agreement and a positive effect of joining any type of collective agreement. The respective losses and gains average -0.6 and +1.1 percent, and with a clear decreasing tendency in both transitions. Again, there is no sign of a close symmetry in the effects of leaving and joining, but there is nevertheless a strong indication that it is better for workers to be associated with covered than non-covered establishments.

[Table 6 near here]

Table 6 further exploits the possibility raised in Table 5 that the analysis is not fully controlling for unobserved establishment heterogeneity. The presumption here is that a sectoral agreement joiner, say, may more closely resemble a sectoral agreement stayer than a sectoral agreement 'never member.' Table 6 thus compares, for the same sample, sectoral agreement joiners with sectoral agreement stayers. Although one may question this new approach – since it seems eminently reasonable to suppose that a sectoral agreement joiner and a sectoral agreement never member share the same beginning-period collective agreement status for non-arbitrary reasons – the strategy is worthwhile pursuing as a form of robustness check on the previous findings. Table 6 thus changes the counterfactuals not only for sectoral agreement changers but for all other coverage transitions as well.

And indeed the results are quite striking. Thus, even if it is accepted from Table 5 that leaving a sectoral agreement has a negative impact on worker wages (taking therefore as a comparison group the subset of always covered), it can be seen from the first row of Table 6 that the course of wage development for workers whose establishments left a sectoral agreement is nevertheless comparatively more favorable than is the case where workers are in an establishment never covered by any type of agreement. Relative to the latter group, there is indeed an *average* gain of 1.0 percent in favor of the former.

The results in the second row of the table are more mixed, with the estimated effect for 2008-2010 indicating that whenever an establishment switches from a sectoral agreement to a firm-level agreement, worker wages go up at higher rate than the wages of those workers in establishments always covered by a firm agreement. This seems consistent with the evidence in Table 4 of a positive gap favorable to sectoral agreements relative to firm agreements. The condition for this interpretation is the assumption that sectoral agreements have a long-lasting effect on wage developments, one that cannot be totally offset even after two years.

The results for sectoral agreement *joiners* in the third row follow the same script: whenever an establishment joins a sectoral agreement, wages presumably go up (based again on the evidence provided in Table 5) but by less than would have occurred had the workers been in an establishment that remained consistently covered by a sectoral agreement over the corresponding sample period. Indeed, the loss amounts to 1.1 percent, on average.[9] In turn, the results in the fifth row suggest that firm agreements have some long-lasting effects, too, as workers in establishments leaving firm agreements continue to receive higher wage increases than their counterparts in never covered plants. For its part, the evidence from the sixth row is more mixed, with a positive effect in the first sub-period and a negative effect in the second.

Next, turning to the aggregate category (i.e. coverage under any type of collective agreement), the seventh row shows that although the evidence from Table 5 would lead us to expect a fall in wages after an establishment leaves a collective agreement of either type, it remains the case that the wage change will still be comparatively more favorable than that obtained by workers in establishments never covered by a collective agreement. Indeed, an average wage gain of 0.9 percent is anticipated as compared with the negative average value of -0.6 percent recorded in Table 5.

Conversely, while workers in an establishment joining a collective agreement are expected to have, say, a 1 percent increase in their wage over a period of two years relatively to those in an establishment never covered by any form of collective agreement (see the last row

[9] The results in the fourth row of the table suggest that joining a sectoral agreement from an initial state of having a firm agreement is more favorable to wage development than being always covered by a sectoral agreement. This finding contradicts the estimates in the fourth row of Table 5. The probable reason is sample size which is quite different in the two experiments, likely invalidating the comparison.

of Table 5), the corresponding results with the different counterfactual in the last row of the Table 6 offer a more qualified story. They show that the wage increase for joining plants **is** comparatively smaller than the wage increases received by workers in those always covered establishments. If anything, there is an increasing gap in this regard, amounting to some -1.3 percent by the end of our sample period in 2008-2010.

6. Conclusions

That over the last two decades German collective bargaining coverage has declined, and that the trend persists, is beyond dispute. Much less clear-cut, however, is the impact of this decline on wage development. Indeed, the literature lacks a critical value: an updated estimate of the union/collective bargaining premium. This lacuna provided the motivation for the present treatment.

Whatever the reasons behind the erosion of collective bargaining coverage, there is little reason to anticipate an elevated union wage gap, since unions should have become weaker rather than stronger (independent support for which proposition is provided by Hirsch and Schnabel, 2011). As a matter of fact, joining any type of agreement from a position of non-coverage has proven decreasingly favorable to wages, while the reverse transition has become decreasingly unfavorable. On the other hand, and looking at the two types of collective agreements separately, leaving sectoral agreements to non-coverage does involve losses (albeit decreasing), while joining a sectoral agreement from non-coverage entails wage gains at a slightly increasing rate. The concatenation of these results obviously implies that workers in establishments that have switched to firm agreements from non-coverage are gradually losing the wage advantage.

Even if the results of this article are not directly comparable with those of previous studies because of differences in methodology, they are consistent with the findings of studies seeking to tackle the causality issue in that the collective bargaining premium is smaller than more conventional estimates. More importantly of course, and unlike previous studies, the present treatment is able to chart *movements* in the collective bargaining premium over the course of the first decade of the 2000s. And in terms of broad movements into and out of collective bargaining, these changes are in the main consistent with the decline in union influence implied by diminishing coverage over that interval.

That being said, the transitions between firm and sectoral collective agreements do not seem to offer any clear-cut conclusions, with indications that establishments switching from firm to sectoral agreements tend increasingly to register wage gains, while at the same time switching from sectoral to firm agreements seems also to be increasingly beneficial. This apparent contradiction seems to be due to the lack of comparability in the selected estimation samples, which is not altogether surprising given the non-experimental nature of our exercise.

More interesting are the results generated by the reverse counterfactuals. Here, the most important finding is that although we generally expect workers to have higher wages after their establishments join a collective agreement, and lower wages after leaving, the gains/losses tend to be smaller (in absolute terms) if one compares the treated group (i.e. joiners or leavers) with the initially selected groups of never members or always members, rather than with always members and never members, respectively. As regards the leavers this finding confirms the presence of some persistence in the effects of collective bargaining coverage. Against this backdrop, we distinctly prefer our two-year estimates to any others based on a shorter interval.

In sum, notwithstanding asymmetry and sensitivity to particular control groups, the evidence reported in this article supports the idea that the coverage premium is modest and has been subject to modest changes. As a matter of fact, in none of the experiments carried out in this study is the coverage premium outside the -1.5/+2.4 percent range. Given this rather narrow margin in the light of the variety of treatment and control groups, it also seems to us unlikely that issues related to the endogeneity of collective agreement decisions are playing a decisive role in our results.

This does not mean that a search for idiosyncratic factors might not prove useful. A case in point might be a change in management ethos. This is one intriguing research area for the future. Another is the much broader theme of whether the distinct decentralization that we can observe within sectoral collective bargaining has had a chilling effect on exits at the same time as the contract premium has declined.

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Table 1
Longitudinal Pattern of Observed Workers

2000-2002				
Year of observation			Number of workers	Number of establishments
2000	2001	2002		
Yes	Yes	Yes	721,321	6,279
Yes	Yes	No	327,829	6,687
Yes	No	Yes	48,503	4,210
Yes	No	No	549,528	9,192
No	Yes	Yes	127,890	5,207
No	Yes	No	142,360	5,972
No	No	Yes	491,841	7,236
			Total=2,409,272	
2008-2010				
2008	2009	2010		
Yes	Yes	Yes	627,027	6,858
Yes	Yes	No	260,129	6,649
Yes	No	Yes	77,968	3,901
Yes	No	No	461,133	9,060
No	Yes	Yes	98,996	5,563
No	Yes	No	93,982	5,715
No	No	Yes	288,995	7,652
			Total=1,908,231	

Note: A given worker necessarily populates one of the seven distinct patterns, but their establishments are not necessarily distinct.

Table 2
Longitudinal Pattern of Workers Observed at Least Twice Over the Observation Window

Workers with three consecutive observations					
	Year				
Profile	2000	2001	2002	Number of workers	Number of establishments
1	A	A	A	716,844	5,222
2	A	A	B	2,168	1,441
3	A	B	B	2,283	1,680
4	A	B	C	26	68
	2008	2009	2010		
1	A	A	A	623,732	6,081
2	A	A	B	1,524	1,122
3	A	B	B	1,754	1,291
4	A	B	C	17	51
Workers with two consecutive observations					
	2000	2001	2002		
5	A	A		325,473	6,176
6	A	B		2,356	1,735
7		A	A	126,439	4,754
8		A	B	1,451	1,207
	2008	2009	2010		
5	A	A		258,710	6,282
6	A	B		1,418	1,134
7		A	A	98,133	5,233
8		A	B	863	826
Workers with two non-consecutive observations					
	2000	2001	2002		
9	A		A	43,142	2,672
10	A		B	3,695	1,869
	2008	2009	2010		
9	A		A	74,832	2,799
10	A		B	2,092	1,202

Note: A, B, and C are establishment identifiers.

Table 3
Two- and One-Year Establishment Collective Bargaining Transitions

One-year transitions	
	t+1=2001

t=2000	Anycb=0	Anycb=1	Total
Anycb=0	2,351	441	2,792
Anycb=1	424	3,215	3,639
Total	2,775	3,656	6,431
	t+1=2002		
t=2001	Anycb=0	Anycb=1	Total
Anycb=0	2,046	270	2,316
Anycb=1	405	2,548	2,953
Total	2,451	2,818	5,269
	t+1=2009		
t=2008	Anycb=0	Anycb=1	Total
Anycb=0	3,385	412	3,798
Anycb=1	413	2,931	3,343
Total	3,798	3,343	7,141
	t+1=2010		
t=2009	Anycb=0	Anycb=1	Total
Anycb=0	3,340	106	3,446
Anycb=1	323	2555	2,878
Total	3,663	2,661	6,324
Two-year transitions			
	t+2=2002		
t=2000	Anycb=0	Anycb=1	Total
Anycb=0	1,829	301	2,130
Anycb=1	422	2,415	2,837
Total	2,251	2,716	4,967
	t+2=2010		
t=2008	Anycb=0	Anycb=1	Total
Anycb=0	2,776	223	2,999
Anycb=1	378	2,280	2,658
Total	3,154	2,503	5,657

Note: Anycb denotes the presence, or otherwise, of any collective bargaining – either sectoral or firm-level bargaining.

Table 4
OLS Wage Regressions, Summary Results

	Collective bargaining argument		
	Dummy variable equal to 1 if	Dummy variable equal to 1 if	Dummy variable equal to 1 if

	worker is in an establishment covered by a sectoral agreement; 0 if the establishment is not covered by any type of agreement			worker is in an establishment covered by a sectoral agreement; 0 if the establishment is covered by a firm-level agreement			worker is in an establishment covered by any type of collective agreement; 0 otherwise		
2000-2002									
	2000	2001	2002	2000	2001	2002	2000	2001	2002
δ	0.070***	0.075***	0.070***	0.018***	0.021***	0.022***	0.064***	0.069***	0.063***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
N	769,989	593,733	637,147	756,738	596,214	688,990	867,881	680,348	787,919
2008-2010									
	2008	2009	2010	2008	2009	2010	2008	2009	2010
δ	0.091***	0.115***	0.140***	0.035***	0.040***	0.039***	0.081***	0.102***	0.125***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
N	656,425	485,264	467,885	677,988	472,299	427,984	818,831	587,493	540,864

Notes: For each cross-section the fitted model is given by $y_i = Z_i\beta + \delta U_j + \omega_i$ [see model (1) in the text]. Standard errors are given in parenthesis.
*** denotes statistical significance at the .01 level.

Table 5

Estimates of the Collective Bargaining Premium, Two-Year Differences (2000-2002 and 2008-2010), Job Stayers

Sample				*Period: 2000-2002*		*Period: 2008-2010*	
Experiment	*Treatment and control groups*	*Collective bargaining status in t-2*	*Collective bargaining status in t*	δ	*N*	δ	*N*
Scb leavers vs. Scb stayers	*Treated group (leavers)*	*Scb=1*	*Anycb=0*	-0.007*** (0.0017)	375,397	-0.004** (0.0018)	289,320
	Control group (stayers)	*Scb=1*	*Scb=1*				
	Treated group (leavers)	*Scb=1*	*Fcb=1*	-0.004** (0.0014)	378,843	0.008*** (0.001)	295,414
	Control group (stayers)	*Scb=1*	*Scb=1*				
Scb joiners vs. Scb never members	*Treated group (joiners)*	*Anycb=0*	*Scb=1*	0.007*** (0.0017)	46,000	0.011*** (0.002)	66,228
	Control group (never members)	*Anycb=0*	*Anycb=0*				
	Treated group (joiners)	*Fcb=1*	*Scb=1*	0.010*** (0.0019)	57,847	0.023*** (0.002)	86,543
	Control group (never members)	*Fcb=1*	*Fcb=1*				
Fcb leavers vs. Fcb stayers	*Treated group (leavers)*	*Fcb=1*	*Anycb=0*	-0.009*** (0.0024)	54,849	-0.007** (0.003)	83,443
	Control group (stayers)	*Fcb=1*	*Fcb=1*				
Fcb joiners vs. Fcb never members	*Treated group (joiners)*	*Anycb=0*	*Fcb=1*	0.024*** (0.0019)	44,297	0.002 (0.002)	64,992
	Control group (never members)	*Anycb=0*	*Anycb=0*				
Anycb leavers vs. Anycb stayers	*Treated group (leavers)*	*Anycb=1*	*Anycb=0*	-0.009*** (0.0013)	447,074	-0.003** (0.002)	393,165
	Control group (stayers)	*Anycb=1*	*Anycb=1*				
Anycb joiners vs. Anycb never members	*Treated group (joiners)*	*Anycb=0*	*Anycb=1*	0.014*** (0.0013)	50,956	0.007*** (0.002)	69,194
	Control group (never members)	*Anycb=0*	*Anycb=0*				

Notes: The fitted model is given model (3) in the text. Anycb is a dummy variable equal to 1 if a worker is in an establishment covered by any type of collective agreement, 0 otherwise; Scb (Fcb) is a dummy variable equal to 1 if a worker is in an establishment covered by a sectoral (firm) agreement, 0 otherwise. Standard errors are given in parenthesis.

***,**,* denote statistical significance at the .01, .05, and .10 levels, respectively.

Table 6
Estimates of the Collective Bargaining Premium, Two-Year Differences (2000-2002 and 2008-2010) but with Different Counterfactuals, Job Stayers

Sample				*Period: 2000-2002*		*Period: 2008-2010*	
Experiment	*Treatment and control groups*	*Collective bargaining status in t-2*	*Collective bargaining status in t*	δ	*N*	δ	*N*
Scb leavers vs. Scb never members	*Treated group (leavers)*	*Scb=1*	*Anycb=0*	0.009*** (0.002)	46,317	0.011*** (0.002)	69,656
	Control group (never members)	*Anycb=0*	*Anycb=0*				
	Treated group (leavers)	*Scb=1*	*Fcb=1*	-0.001 (0.001)	61,863	0.010*** (0.002)	93,589
	Control group (never members)	*Fcb=1*	*Fcb=1*				
Scb joiners vs. Scb always members	*Treated group (joiners)*	*Anycb=0*	*Scb=1*	-0.009*** (0.002)	375,080	-0.013*** (0.002)	285,892
	Control group (always members)	*Scb=1*	*Scb=1*				
	Treated group (joiners)	*Fcb=1*	*Scb=1*	0.009*** (0.002)	374,827	0.021*** (0.002)	288,368
	Control group (always members)	*Scb=1*	*Scb=1*				
Fcb leavers vs. Anycb never members	*Treated group (leavers)*	*Fcb=1*	*Anycb=0*	0.004* (0.002)	42,749	0.007*** (0.002)	65,604
	Control group (never members)	*Anycb=0*	*Anycb=0*				
Fcb joiners vs. Fcb always members	*Treated group (joiners)*	*Anycb=0*	*Fcb=1*	0.014*** (0.002)	56,397	-0.015*** (0.003)	82,830
	Control group (always members)	*Fcb=1*	*Fcb=1*				
Anycb leavers vs. Anycb never members	*Treated group (leavers)*	*Anycb=1*	*Anycb=0*	0.007*** (0.001)	49,725	0.010*** (0.001)	73,234
	Control group (never members)	*Anycb=0*	*Anycb=0*				
Anycb joiner vs. Anycb always members	*Treated group (joiners)*	*Anycb=0*	*Anycb=1*	-0.002 (0.001)	448,305	-0.013*** (0.002)	389,124
	Control group (always members)	*Anycb=1*	*Anycb=1*				

Note: See notes to Table 5.